

Responses to comments on the draft literature review entitled, “Stress in Mammals: The Potential Influence of Fishery–Induced stress ion Dolphins in the Eastern Tropical Pacific Ocean”

Inter-American Tropical Tuna Commission

3 February 1999

(Responses are numerated to correspond to paragraphs in the IATTC’s 3 February letter commenting on the literature review)

1. re: “The paper argues that it is plausible...”

The goal of the draft review paper is to make an objective assessment of the available literature regarding stress in mammals, not to argue for or against the possibility of stress affecting dolphins involved in the eastern tropical Pacific (ETP) purse-seine fishery. In fact, the review begins by stating in the introduction that the International Dolphin Conservation Program Act requires that a review of relevant stress-related research be completed by the National Marine Fisheries Service, and that the manuscript addresses that requirement by reviewing current knowledge of stress in mammals. The manuscript focuses on scientific information regarding physiological responses to stress and the potential consequences of fishery-induced stress to dolphins involved in the ETP tuna-purse-seine fishery. The overall objective of the review is to provide a context for future scientific findings by describing what is known about physiological and behavioral responses to stress in mammals and relating that information to the chase and encirclement of dolphins in the ETP fishery. The available scientific literature supports the possibility that the fishing operations involved in the ETP fishery is likely to cause stress to the dolphins involved. The available literature was surveyed uniformly, without selective bias for papers that might or might not support a hypothesis of fishery-induced stress in dolphins involved with the fishery.

2. re: “The definitions of stress and stressors...”

The review provides a fairly extensive background section on the concept of stress and stress theory in an attempt to convey the complexity of stress and the

physiological stress response in mammals. The definition of stress as stated in the review (“a state of threatened homeostasis”) is a generally accepted operative definition (see Rivier and Rivest, 1991; Chrousos and Gold, 1992; Johnson et al., 1992).

To help clarify the confusion noted by the IATTC, I have added a brief definition of homeostasis on page 9 of the review.

The statement on page 15 of the review;

“The stress response functions to return the body to homeostasis under conditions of acute, abbreviated stress. Thus, when the duration of a stressor is limited, the physiological effects of stress are beneficial and have no adverse consequences. However, some conditions can lead to dysregulation or pathophysiology resulting from stress (Chrousos and Gold, 1992). Chronic or severe stressors can cause responses that lead to pathologies and the exhaustive state described by Selye (1936).”

merely repeats, but in no way contradicts, what was said on page 9 of the review;

“...an organism’s responses to stress are adaptive (acting to regain homeostasis) and can be both physiological and behavioral. The consequences of stress can be adverse, however, when the organism is unable to successfully regulate the stress response.”

Thus, if the consequences [of a stressor] are not adverse, it means that the organism’s physiological response to stress acted to regain homeostasis. The hypothetical stressor did indeed threaten homeostasis, but the stress response addressed the threat and restored the body to its normal state. The stress did, however, occur as is explained in the background section on Stress in Mammals (section I.C).

3. re: (1) “Marine mammals appear to have some differences in stress response...”

As is described in the review (section I.C.4.f, Physiological Stress in Mammals) marine mammals do differ from terrestrial mammals in the profile of their cortisol and aldosterone responses to stress, and do exhibit a change in thyroid hormone balance in response to stress. These differences in the cetacean stress response are significant, but do not fundamentally alter their pattern in the basic mammalian response to stress.

In fact, as noted on page 19 of the review, cetaceans appear to have a modest cortisol response to stress. Interestingly, unpublished data indicates that far less circulating cortisol is bound in cetaceans when compared to terrestrial mammals. Thus, a greater amount of circulating cortisol is physiologically active in cetaceans. It appears

that small changes in cortisol in cetaceans result in unambiguous changes in the blood constituents responding to cortisol.

Because aldosterone increases in response to stressors in cetaceans, it may provide an additional reliable blood indicator of the stress response in these animals. These factors do not affect the conclusion of the review.

re: (2) “It should be cautioned that the other cetacean studies typically involve coastal populations...”

Actually, many of the studies of stress responses in cetaceans have involved captive animals. The study of St. Aubin *et al.* (1996) did involve a coastal population of bottlenose dolphins and this is now noted in the text of the review (page 25). However, it is very difficult, if not impossible to predict the potential difference in responses to stress between coastal and pelagic dolphins. Coastal dolphins are likely to have more experience of spatial boundaries and perhaps more frequent experience with vessels.

The potential of habituation of dolphins in the ETP to chase and capture procedures is addressed in the review. However, the potential for an anticipatory response to impending capture is also addressed. In addition, the literature review discusses the ways in which neonatal/juvenile animals may be particularly at risk. Threat to younger animals would not likely be ameliorated by acclimatization as they would not be experienced enough to have achieved it.

re: (3) “It should be explained that the levels of stress involved in these other cetacean studies are greater than experienced typically by dolphins in the EPO.”

It is not possible to say that “the levels of stress in other cetacean studies are greater than that experienced by dolphins in the EPO.” In fact, there are multiple stressors encountered simultaneously by dolphins involved in the ETP fishery (see Table 1 of the review), and it is quite possible that the stress of chase and capture of dolphins in the ETP tuna fishery is greater than that of dolphins in most of the studies detailed in the review.

4. re: “The conclusion that...”

I have added to the discussion section (page 56) the information that spotted and spinner dolphins appear to be largely nocturnal feeders. The review states that,

“One additional potentially significant aspect of search, chase and encirclement of dolphins in the ETP, is that the inter-species association between dolphins and tuna is disrupted.” (page 56)

and suggests that one important aspect of this may be the loss of a nutritional advantage conferred by the association. I have added a very brief discussion of the ways in which the tuna/dolphin association could provide a nutritional advantage to the dolphins. I have also cited the discussion of daytime increases in dolphin herd sizes by Scott and Cattanch (1998):

“Daytime aggregation [for spotted, spinner, and common dolphins] may be promoted by additional advantages, such as more-efficient searching for prey that will migrate vertically later at night and increased opportunities for mating and information transfer.”

to support the idea that displacement of the dolphins during chase and capture is likely to disrupt these activities.

5. re: “The review also concludes that the separation of herds can also be stressful, and that kin-related groups are likely to be broken up...”

Rather than simply concluding that separation of herds can be stressful, the review states that

“Given the probable importance of social relationships among dolphins and the benefits conferred upon individuals by herding, separation, when it does occur, is very likely to elicit an adrenocortical response in these dolphins.” (page 56)

The review also cites the work of Scott and Cattanch (1998) to support the comment that herd composition can be fluid and there are fluctuating changes in group size and composition. The review emphasizes the importance of kin-related groups, but it seems probable that during the events of chase and capture, some sub-groups will become separated.

6. re: “It is argued that...”

Net entanglement and entrapment under the net canopy are potential components of capture for dolphins in the ETP fishery.

It may be correct that the results of two studies cited (Myrick and Perkins, 1995 and Young et al., 1997) could have been confounded by the possibility that some of the dolphins examined in the studies may have died due to entanglement in the nets. The problem with the respective authors' interpretations in those studies reflect their hypotheses and methods used to examine them. Accordingly, the review urges caution in interpreting the results of Myrick and Perkins (1995) and comments that insufficient information was given to test the validity of the results of Young et al. (1997).

7. re: "In regard to the argument that stress is disrupting reproduction..."

Chivers and DeMaster (1994) is now included in the review (section II.C.3, Stress Induced Changes in Reproductive Function). The Myrick and Chivers (1993) study included only a very small subset of the specimens included in Chivers and DeMaster (1994), whose results may be considered to more accurately estimate biological patterns of the populations examined.

8. re: While the mention of the Myrick and Perkins paper..."

The review discusses Myrick and Perkins (1995) providing insight into its shortcomings and cautioning that the results must be interpreted with caution. No further comment has been added.

9. re: "The section on capture myopathy fails..."

The results of Cowan and Walker (1979) are now included in the section (II.B.3.a) on capture myopathy.

In section ii.C.1.a (Stress-Related Pathologies in Dolphins) the text reads as follows:

"Two studies have investigated the occurrence of stress-related pathologies in specimens of spinner and spotted dolphins recovered from the ETP fishery. In the first study, Cowan and Walker (1979) examined spotted and spinner dolphins killed in the ETP fishery and defined three states of disease: 1. naturally occurring disease, 2. tissue changes resulting from acute responses to terminal events, and 3. subacute pathological conditions that could hypothetically lead to death. Parasitism was found to be the most prevalent cause of naturally occurring disease. Acute tissue changes were attributed to drowning in fishing and research gear, and were described for the lungs, heart, adrenal glands, and spleen.

Cowan and Walker (1979) concluded that there was no substantial evidence of "delayed mortality" (documented in terrestrial mammals suffering from capture myopathy) related to the fishery. However, they noted that several of the dolphins apparently died of massive cardiac reaction to stress and

were documented to have cardiac lesions consistent with those produced in laboratory animals injected with catecholamine and humans thought to have died of stress cardiomyopathy (Cowan and Walker, 1979; and see Cebelin and Hirsch, 1980).” (Pages 39-40)

Cowan and Walker (1979) stated the following:

“Few porpoise examined demonstrated no hemorrhages, petechia, or pulmonary emphysema and were free of external signs of injury. These were presumed to have died of “stress,” a vaguely defined concept in this situation which is thought to be related to a massive cardiovascular reaction to either autonomic discharge or adrenal cortical secretion or both. “Contraction banding” of myocardial fibers and of cardiac conduction fibers was prominent in some animals. This has been well described in man and a variety of other animals in many fatal situations, including drowning, non-specific injury, panic, etc., and may be considered a response to almost any circumstance drastic enough to produce death. Similar lesions may be produced in the laboratory by catecholamine injections...”

I believe the information on this subject, as included in the review, is correct.

Marine Mammal Commission (MMC)
12 February 1999

I have revised the literature review of stress in mammals according to the copy of the draft which was edited by the MMC. These changes included minor typographical errors and suggestions for clarifying certain points.

I have also reread and revised the review to address the statement that in some cases the draft fails to differentiate between stress-related responses that are universal and those that may vary from individual to individual. As noted to me by the MMC, comments written on the draft manuscript returned to the NMFS specifically addressed places in the document that required change to address this concern. These changes have all been made.

I have also added a paragraph to the Discussion as follows:

“Based on the literature from studies of stress in mammals, it is apparent that there will be differences in stress-related responses among species and individuals. Differences in temperament, age, sex, and health condition are all likely to influence the response of dolphins in the ETP to fisheries activities. Differences in experience with the fishery may also influence responses to stress among individual dolphins. Further, each encounter with the fishery will differ and this too will influence the potential response of an individual dolphin.”

Outside Reviewer's Comments

The literature review was sent by the author to the following scientists for peer review:

David. J. St. Aubin, Ph.D.
Director, Research and Veterinary Science
Mystic Aquarium

Daniel F. Cowan, M.D.
Department of Pathology
University of Texas Medical Branch

Joy A. Mench
Associate Professor for Animal Biology
University of California, Davis

Gary P. Moberg, Ph.D.
Professor and Associate Dean for Animal Biology
University of California, Davis

Joseph R. Geraci, D.V.M. (Comments contained with Marine Mammal Commission
Baltimore Aquarium review)

David J. St.Aubin, Ph.D.
22 January 1999

Response to general comment:

I have reread the manuscript to look for areas and topics that might benefit from a discussion of the time-course of changes discussed. In the background section (Stress in Mammals), I attempted to give perspective on the potential responses to stress. In other words, I attempted to convey that some responses to stress are temporary, adaptive and can benefit the organism by returning its body to homeostasis.

I also added a brief comment in the introduction of section II (The Potential for Fisheries-Induced Stress on Dolphins in the ETP)

“In assessing the potential effects of search, chase and capture on dolphins in the ETP, is important to acknowledge that physiological responses to stress may be adaptive and temporary, acting to restore homeostasis. Such physiological changes may be of little consequence to individual dolphins. It does not appear possible to predict the specific level or exact duration of changes in physiological parameters that could become adverse to the health of individual dolphins”. (Page 21)

Response to substantive comments:

re: p. 17, Section d., first sentence. I have corrected this statement to reflect the fact that cortisol, already circulating in the bloodstream, is increased when glucocorticoids are released from the adrenal gland.

re: p. 17, Section d., third sentence. I have included the information that the amount of bound cortisol varies among species. I found the unpublished data to be interesting, and did not, of course, include it at the present time. The information would be valuable for future studies, if it could be published.

re: 24, last paragraph. I have omitted the information regarding methodology (radioimmunoassay).

last complete sentence. I corrected the grammatical error. I rewrote the paragraph to more accurately reflect the results of St. Aubin et al. (1996).

re: p. 35 and p. 57. The results of Cowan and Walker (1979) are now included as a separate paragraph in the treatment of capture myopathy (page 35),

“Cowan and Walker (1979) examined 49 spotted and 16 spinner dolphins killed during routine fishing operations in the ETP fishery. They examined several primary locomotor muscles (Longissimus dorsi and Hypaxial muscles) for both gross pathological or histopathological changes. No evidence of myopathy was detected in the skeletal muscles of the 56 animals examined (Cowan and Walker, 1979).”

I have also edited the paragraph on page 57 as follows:

“Cowan and Walker (1979) found no evidence of capture myopathy from observations of skeletal muscle obtained from dolphins killed in the ETP fishery. This may be evidence that muscle damage does not occur in dolphins as the result of chase and capture. Alternatively, based on information from terrestrial mammals, it seems possible that myopathic changes to skeletal muscle may occur gradually, during and post-capture, as the cumulative effect of systemic changes caused by the syndrome.”

re: p. 40, last paragraph. I rewrote this paragraph including the relevant background information for each of the cases discussed.

“Adrenocortical cysts, which may be another stress-related pathology, have been observed to occur in several cetacean species including in mass stranded Atlantic white-sided dolphins, *Lagenorhynchus acutus* (Geraci *et al.*, 1978); a captive common dolphin (Cartee *et al.*, 1995), stranded beluga whales (Lair *et al.*, 1997) and harbor porpoises, *Phocoena phocoena* (Kuiken *et al.*, 1993). The belugas sampled were exposed to xenobiotics and generally suffering from chronic disease. Adrenocortical hyperplasia was observed in 24 out of 35 stranded belugas, and cortical cysts containing a cortisol concentrated fluid were found in 19 of the whales (Lair *et al.*, 1997). Lair *et al.* (1997) suspected that the adrenocortical cysts might have been stress-related. Harbor porpoises that were also chronically sick and exposed to chlorinated hydrocarbons were observed to have adrenocortical hyperplasia that was interpreted to result from chronic stress (Kuiken *et al.*, 1993). The mass stranded Atlantic white-sided dolphins could be considered to represent a generally “healthy” population, and Geraci *et al.* (1978) determined that the adrenocortical cysts observed in those animals, were probably stress-related.”

re: p. 48, last paragraph. I have rewritten the first sentence accordingly:

“Lactation can also be altered as the result of stress. Prolactin levels, which are generally increased during lactation, affect milk production in mammals and can also be increased in response to stress.”

re: p. 59, first paragraph. Plans for necropsy sampling of dolphins killed in the ETP tuna purse-seine fishery include collecting basic information on life history, health and disease, parasite load, contaminant load, and nutritional state. Plans also include sampling for studies of immunology. I have added a comment to the effect that information on the immune status of the animals could be assessed given that background information regarding life history and health status are known.

I have corrected all grammatical and typographical errors noted.

Daniel F. Cowan

25 January 1999

I have addressed all of the comments noted by Dr. Cowan.

Joy A. Mench

28 January 1999

A mention of catecholamines is included under the topic of “other physiological agents” in the background section of the review (Section I.C.4). As noted by Dr. Mench, some potential effects of catecholamines are included in the review.

Dr. Lay’s paper from the scientific conference, “The Biology of Animal Stress,” University of California, Davis, August 16-19, 1998 is not available. However, the topic of exposing pregnant females to stress is discussed under the subject of hyperthermia (Section II.B.3.b) and is also mention under the topic of reproduction (Section II.C.3) in the review.